forming thirty-five perfect Fifths, which, by the contrivance of using the eighth Fifth down as the major Third, into the place of which its finger-key is placed, gives very free play for modulation in all directions with perfect uniformity, although of course slightly imperfect intonation.[1]

To this system of perfect Fifths and major Thirds identified with eight Fifths down, we must attach all the other contrivances for reducing the number of notes necessary to tertian harmony, without seriously offending the ear. The practicability of arranging any number of dozens of such notes in the Octave, up to at least seven dozen, so that they should be entirely under the command of the performer, be fingered precisely in the same way in all keys, and have a style of fingering which is of about the same difficulty as that for three sharps or three flats on the piano, has been proved to demonstration by the "generalised keyboard" of Mr. Bosanquet, exhibited at the Loan Collection, for his enharmonic harmonium, and, up to four dozen finger keys to the Octave, to the Musical Association upon his enharmonic Organ.[2] This keyboard is quite a triumph of ingenious construction, founded on rigorously scientific principles, for the practical solution of an apparently insoluble problem.

The modifications of the perfect Fifth system (to which Mr. Bosanquet seems much inclined, *op. cit.*, p. 57) depend on the discovery that fifty-three perfect Fifths exceed thirty-one Octaves by only about $\frac{36}{1000}$ of an equal semitone, or very nearly $\frac{2}{11}$ of a comma. Helmholtz proposed to reduce every Fifth by $\frac{1}{88}$ of a comma. This would make fifty-three of the flattened Fifths to be about $\frac{1}{10}$ of a semitone less than thirty-one octaves, too large an interval for good ears not to perceive, being nearly half a comma, but then all his Fifths would be audibly perfect, and all his major Thirds absolutely perfect. Mr. Bosanquet endeavoured to tune a stop on his enharmonic organ in that way, but the effect with stopped pipes did not repay the immense trouble of tuning (*ibid.*), which cannot be truly effected without much mechanical assistance, and is therefore generally impracticable.

The great difficulty of tuning is also an objection which applies to Mr. Bosanquet's own proposal to divide the Octave absolutely into fifty-three parts (see *op. cit.*, p. 56). This would flatten the Fifth still less, but of course would also make the major Thirds nearly as flat as those in the system of perfect Fifths, from which his differ only by about $\frac{1}{44}$ of a comma. It is not likely that Mr. Bosanquet has been able to tune to such a degree of accuracy. And as the object of the division of fifty-three is only to modulate *ad infinitum*, such accuracy is needless for general purposes, for which forty-eight perfect Fifths (or, as I believe, six sets of eight perfect Fifths, differing by perfect major Thirds from each other, and hence comparatively easy to tune by Fifths and check by Thirds), would fully suffice.[3]

The instruments exhibited in the Loan Exhibition and the others indicated in the preceding lines may, therefore, be said to have practically solved the difficulty of tertian harmony on instruments with fixed tones, and they have even approached to a solution for septimal harmony (which uses the harmonic Seventh; see Bosanquet, *op. cit.*, p. 41). Voices in part music, when unaccompanied, must sing in just tertian or even septimal harmony, but when accompanied they will inevitably follow the instrument. Violinists can do what they like, but are too much inclined to Greek intonation, which is all very well by itself, but which the performer should learn to modify by something better than a rule of finger, in double-stopped passages and part music. With the bass the comma stop may be made effective to a great extent, and Dr. Stone is trying what he can do with the oboe and clarinet (*op. cit.*, p. 35), so that there are some hopes of improving even the orchestra. Enough, at least, has been done on the instruments mentioned and in the practice and system of study of the Tonic Solfaists (Helmholtz, p. 640) to show that it is practically possible greatly to improve musical intonation.

ALEXANDER J. ELLIS

[1] The notes may be considered to be those in Gen. Thompson's second column continued up to A*sh.* and down to A*ffffl.* (or A quadruple flat), the names being altered to those of the finger-keys corresponding to those on the ordinary piano, so that no more sharps and flats are used than in ordinary notation. Unfortunately the bellows and some parts of the mechanism were injured in the carriage, and hence the full effect could not be appreciated. There were two extra rows of keys to bring out Herr Appunn's favourite minor Third, 16 : 19, which is slightly closer than that on the piano, and very effective in certain cases, but the fingering for these was new and difficult, and could not be considered practical.

[2] The first, with eighty-four notes to the Octave, is figured in Mr. Bosanquet's book (*op. cit.*, p. 23), where it is fully described, and is also explained at length in my Helmholtz, pp. 692–696. In both books the builder's name and address are given, with his prices for compasses of two to seven dozen keys per Octave. Such instruments are indispensable for the scientific cultivation of music. It is also suited for mean-tone intonation.

[3] See the full explanations in my Helmholtz, pp. 656, 657, 783. Tuning by perfect intervals is the only system practicable without mechanical assistance. But even to tune the thirty-five Fifths of Appunn is impossible by ear alone. I find tuners have a difficulty with only eight notes, forming seven successive Fifths. But when the next eight notes are taken as major Thirds to these, all verified by forming Fifths with each other and producing correct differential tones with the original set, we may hope for some correctness. The only really satisfactory way of tuning is by calculating the pitch of each note, and then causing a set of forks, or Appunn reeds, to be constructed, each too flat by four vibrations, which can be done exactly by Appunn's tonometer. After tuning a note roughly to one of these, sharpen it till it beats four times in a second with the standard. Any temperament, even Helmholtz's, the best in existence, can thus be easily and perfectly realised note by note.

ON PHOTO-CHEMICAL PROCESSES IN THE RETINA

IN an article which lately appeared in NATURE (vol. xv., p. 308), I gave an account of certain very remarkable discoveries made by Prof. Kühne, of Heidelberg, which added additional interest to the startling announcement contained in a recent communication made by Prof. Boll, of Rome, to the Berlin Academy, to wit, that the external layer of the retina is, during life, of a purple colour, which disappears at death, but which is, during life, continually being bleached by the action of light. In my first communication I stated that the account of Boll's researches which I was able to give, was only quoted at second-hand from Kühne's paper, as the number of the *Proceedings* of the Berlin Academy containing Boll's communication[1] had not yet reached Manchester. Having now had the opportunity of reading that communication, I am able to state that the summary of it contained in my first article was correct in every particular. As the paper is, however, one of peculiar importance I propose, with the concurrence of the Editor of NATURE, to insert a *verbatim* translation of it in next week's number of NATURE.

It is with great surprise that I have heard that the prominence given to Prof. Kühne's researches on the "Vision-purple" in my article in NATURE has given some pain to Prof. Boll, who probably feels some disappointment in not having been allowed to remain in sole possession of the promising field of research upon which he had entered. It is with still greater surprise, however, that I have read the remarks which D . Warlemont, editor of the *Annales d'Oculistique*, has added to the literal translation of my article which he has published in that journal,[2] and which follows a brief abstract of Boll's paper.

"Nous appelons toute l'attention de nos lecteurs sur les deux articles qu'ils viennent de lire, et qui signalent une découverte propre à révolutionner la physiologie de la rétine, à renverser quelques unes des idées reçues, à en affirmer beaucoup d'autres. *Tout le mérite de la découverte de la coloration propre de la rétine appartient à M. le professeur Boll, avec toutes ses conséquences,* dont M. Kühne nous parait s'être prématurément emparé. M.

[1] Monatsbericht der königlichen preussischen Akademie der Wissenschaften zu Berlin, November, 1876. Gesammtsitzung vom 23 November, 1876, S. 783–787.

[2] *Annales d'Oculistique*, tome lxxvii., Janvier-Février 1877, pp. 78-81.

Boll avait évidemment entrevu toutes ces conséquences, et il eut été de bon gout, nous semble-t-il, de lui laisser le temps de les dérouler à l'aise. C'est donc sans droit que nous voyons déjà, dès à présent, la presse parler, à propos de ce fait, des 'découvertes de MM. Boll et Kühne' et le nom de ce dernier associé à celui du *seul inventeur*.

"Deux gamins suivaient un trottoir; l'un d'eux sifflaient un air, dont il n'était qu'à la moitié, quand le second se mit à la continuer: 'Une autre fois,' lui dit le premier le regardant très mécontent, 'tu voudras bien commencer toi-même.'"

If I quote the above sentences it is to show that they are as much opposed to truth as they are to the interests of science, or as they are repugnant to good taste. When a scientific man has published a discovery it is to the interest of the scientific world that all who will or can should be at liberty to repeat the experiments or observations which led to it; if other great discoveries are made by the new labourers it is to the interest of science that they be published.

In the particular case in point it would appear that Prof. Boll re-discovered (and, what is more, *appreciated the full value* of) a fact which had really been observed by some others (Leydig in 1857, and Max Schultze in 1866), but which had certainly not become part and parcel of the common stock of scientific knowledge, viz., that the rods of the retina are red, he observed that under the circumstances of his own experiments the colour faded at death, and arrived at the false conclusion that the colour was a function of the vital condition of the retina. He, however, observed the remarkable action of light in modifying the colour. "During life," he announced in his paper, "the peculiar colour of the retina is continually being destroyed by the light which penetrates the eye. Diffuse daylight causes the purple tint of the retina to pale. The more prolonged, dazzling action of the direct rays of the sun entirely destroys the colour of the retina. In darkness the intense purple colour is again restored. This objective alteration of the peripheral structures of the retina brought about by the rays of light undoubtedly occurs in the act of vision." This was the great discovery which Boll made, and with which his name will ever remain honourably connected. Although, however, he had been in full possession of the facts in the month of June of last year, when he demonstrated them to Professors Du Bois-Reymond and Helmholtz, and only published his paper in November, he did not succeed in making the discoveries with which, justly, the name of Kühne is now associated. Kühne showed that if the retinal purple is usually destroyed at death, the result is attributable to the action of light, persistence of the colour being by no means necessarily connected with the living condition of the retina. In his beautiful and far-seeing discovery of the true function of the retinal epithelium cells as restorers of the vision purple, he was fortunate enough to make a discovery which it would be very bold for any one—even for Prof. Boll—to say he would have made, had time and opportunities been granted. In saying that Boll had discovered everything referring to the vision purple, M. Warlemont shows that he has not appreciated the fact that two great discoveries have been made, the second supplementing the first, and actually needed in order that the significance of the first should be appreciated.

But I trust that the readers of NATURE do not think that I wish to depreciate the researches of Prof. Boll whilst I act as the champion of one who needs no champion, seeing that he illustrates in himself the truth of the adage, "le grand mérite est toujours probe."

Prof. Boll must reflect that great discoveries are rarely completed by one man, and that it is no shame, and should be no cause of sorrow, to the true man of science, if the conception which he has tried to develop and which he has almost raised to the position of a truth by his own work, receives its final development through the strivings of a fellow-worker.

Abandoning the polemical discussion upon which I felt myself almost compelled to enter, I would give an account of the most recent results obtained by Kühne on the "Vision Purple," and published by him in the *Centralblatt für die medicinischen Wissenschaften* for March 17 (No. 11).

The purple colour of the retina is now shown to depend upon the presence of a substance which can be dissolved and separated in the solid form. The only solvent of the vision-purple as yet known is bile, or a pure glyko-cholate. The filtered, clear solution of the vision-purple is of a beautiful carmine-red, which, when exposed to light, rapidly assumes a chamois colour, and then becomes colourless. As long as it is at all red the solution absorbs all the rays of the spectrum, from yellowish-green to violet, allowing but little of the violet, but all the yellow, orange, and red rays to pass. Accordingly, bloodless retinæ spread out and placed in the spectrum, between green and violet appear grey or black.

Kühne has exposed retinæ in different parts of a spectrum (obtained by allowing the sun's rays between eleven and one o'clock to fall through a slit 0·3 mm. wide upon a flint glass prism) in which Fraunhofer's lines were shown in great number and with great distinctness, and he has ascertained that in the yellowish green and green regions the vision-purple is bleached most rapidly; the action is less in the bluish green, blue, indigo, and violet; it is still perceptible in the orange and yellow, but not in the red or ultra-violet regions.

March 24 ARTHUR GAMGEE

OUR ASTRONOMICAL COLUMN

THE CAPE ASTRONOMICAL RESULTS, 1871–1873.—Mr. Stone has just circulated the results of meridional observations of stars made at the Royal Observatory, Cape of Good Hope, in the years 1871-1873. His present object has been not so much to furnish extremely accurate places of principal southern stars as to supply reliable positions of stars down to the seventh magnitude within 15° of the South Pole, and it is considered that this volume contains all Lacaille's stars in this region of the sky, and very nearly all sevenths not observed by him. It is the "first published instalment of the materials collected for the projected Catalogue." The separate results for mean R.A. and N.P.D. are given, with catalogues of places for the commencement of each year, the whole number of stars observed being about 1,400. Bessel's reduction constants are appended. This form of publication is perhaps sufficiently ample in the present day, though Mr. Stone alludes to a desire expressed by some astronomers to see the Cape observations printed in detail in the same manner as the Greenwich observations, a plan hardly practicable with the limited staff at his disposal, and which would involve very slow progress of the work with the resources of the Cape press. We are inclined to think that Mr. Stone exercises a wise discretion in limiting his volume to its present form, and thus assuring its comparatively early distribution in the astronomical world. As it is the volume is not produced without a considerable expenditure of time in the routine work of the reductions by the director himself.

VARIABLE STARS.—Mr. J. E. Gore, of Umballa, writes with reference to several stars which may prove to be variable:—(1) Lalande 14088 (Canis Major); this star was observed by Lalande, March 2, 1798, but the magnitude was not registered. It is marked of the ninth magnitude only on Harding's Atlas, but at the beginning of February in the present year Mr. Gore found it a little brighter than the sixth magnitude Lalande 14105, closely south of it, and "decidedly reddish." Argelander observed this star on December 23, 1852, and rated it 6 m. He is